Competition and Product Misrepresentation*

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Abstract

This paper examines the effect of competition on product quality when product quality is unobserved before purchase. Using a dataset that records the actual broadband internet speed consumers receive as well as the speed the provider claims is being delivered, I find that an additional broadband competitor raises the ratio of actual to claimed speeds for incumbents by between 23 and 32% within the first 6 months, but that this effect attenuates after 18 to 30 months. This increase is due to improvements in the actual speed, and not just reductions in the claimed speed. I recover the causal effect of competition on product misrepresentation by leveraging the launch of a broadband-capable satellite in mid-2012 and exploiting exogenous variation in suitability for satellite internet across U.S. counties. I provide suggestive evidence that the reduction in firms' strategic misrepresentation of their products led to reduced misallocation of consumers across internet plans.

Keywords:

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1 Introduction

When making consumption decisions, consumers often do not perceive the true characteristics of a good until they actually purchase it. Ex ante, consumers rely on descriptions of product attributes supplied by firms. Recent work on cheap talk models of advertising has shown that when a product's features are unknown before consumption, firms may have an incentive to claim their product is higher quality than it actually is to attract consumers [Gardete, 2013]. This incentive to misrepresent product quality is particularly strong in industries where there is little chance of repeat business or where switching costs are high [Cai and Obara, 2009]. Two natural empirical questions arise from the theory: first, do firms actually strategically misrepresent their

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product characteristics? And if so, in the absence of third-party regulation or monitoring, will competitive forces in the market discipline firms' product claims?

To understand the relationship between claimed and actual product quality, I use a panel dataset of consumer purchases of broadband internet service. The download speed of an internet plan is one of its most important features, but whether an internet service provider (ISP) actually delivers the speed that is paid for is not known until the consumer actually signs the contract and begins service, at which point the cost to cancel service can be substantial. The panel data provides both the nominal speed the ISP claims to be providing as well as the measured speed that is actually being provided, which allows me to directly measure how the difference in claimed and actual product quality responds to covariates.

The paper's main empirical contribution is to recover the effects of competition on the ratio of consumers' actual to claimed download speed. This ratio, henceforth referred to as the *promise ratio*, exhibits substantial variation: across consumers and within an ISP-month, the consumer at the 10th percentile of the ratio receives approximately 77% of the speed they pay for, while the 90th percentile consumer receives 117%. In my preferred specification, I show that competition has a substantial and statistically significant effect on the ratio of actual to claimed speed; the presence of an additional broadband internet competitor offering high speed plans (up to 15Mb/s) increases the promise ratio by 23-32%, although with a full set of ISP-month fixed effects the coefficient is significant at only the 10% level. I show that this result is not driven by reductions in claimed speed.

I next turn to the question of whether strategic misrepresentation matters for economic outcomes. In particular, I look at whether ISPs' strategic noisiness in their plan descriptions induces consumers to pick sub-optimal plans, or whether misrepresentation is simply cheap talk and consumers accurately perceive the true qualities. I use two empirical tests of whether consumers were misallocated across plans: whether consumers on plans that were strategically misrepresented change their average consumption of content after the qualities are improved in response to competition, and whether the same consumers change their plan search behaviour. I find that both tests provide suggestive evidence for the hypothesis that firms' strategic misrepresentation led to sub-optimal plan choice.

My empirical strategy to understand the effect of competition on the promise ratio is a difference-in-differences specification. The main threat to identification is that the entry of competitors in an area might be endogenous to the promise ratio. Since a fall in an ISP's promise ratio may lead to an increased willingness of consumers to switch internet providers, competitors are more likely to enter, which would generate a negative relationship between changes in the promise ratio and the number of competitors. To resolve this issue and establish causation from number of competitors to the ratio, I use the fact that a provider of satellite internet brought a broadband-capable satellite online during 2012. The launch provides an exogenous increase in the number of competitors.

While nominally the launch may have provided an additional competitor for every household, some areas may not be suitable for satellite internet, due to geography, weather, or characteristics of local housing stock. I use the presence of existing satellite customers in a county in 2010 and 2011 to construct cross-sectional variation in which counties are affected by the launch; the intuition is that counties with relatively large ex ante share of satellite customers are the only areas suitable for satellite internet.

An area's suitability for satellite internet depends partly on covariates that are exogenous to short run shocks to local firms' promise ratios. I leverage two sources of cross-sectional variation orthogonal to short run shocks to instrument for whether a county is treated by the satellite launch: variations at the county level in severe weather events, and variations in the housing stock—in particular the fraction of manufactured/mobile homes—across census block groups [Boik, 2017]. The instruments are predictive and have intuitively correct signs in the first stage, and increase the estimates of the effect of the satellite launch on the promise ratio by an order of magnitude. I also validate the weather instrument by showing that thunderstorms indeed have an effect on internet quality using high-frequency testing data.

In the final section I turn to the question of whether misrepresentation leads to consumer misallocation across plans. I construct two tests to address this question, based on how a household's consumption of content (in megabytes or gigabytes) varies with both their actual and promised downloaded speeds. I depart from the instrumented diff-in-diff strategy above as it is no longer the effect of competition I am interested in, but the effect of varying the claimed download speed on households' consumption while keeping actual speeds constant. Both exercises provide suggestive evidence of misallocation, and while neither constitutes a formal test they are consistent with households not being able to achieve their optimum consumption profile due to misrepresentation induces incorrect plan choices.

The remainder of the paper proceeds as follows. I first discuss the industry background, the literature, and the research questions I will take to the data in Section 2. I then explain the industry background, the data and the construction of my variables in Section 3. Section 4 covers the estimation and analysis, and Section 6 concludes.

2 Literature and Research Questions

Policy makers have long worried whether firms sufficiently inform consumers about payoff-relevant product attributes. In the U.S. especially, federal agencies such as the Federal Department of Agriculture and Federal Aviation Authority have broad mandates to enforce mandatory disclosure of product attributes ranging from calorie content to airline on-time performance. In the absence of regulation, whether firms will disclose information about their products, what they will choose to disclose if they do, and how the incentives to disclose interact with market structure, have all generated substantial debate among economists but only limited validation in the data, necessitating the empirical approach taken in this paper.

Theoretical work on how competition affects the incentive to honestly signal product attributes offers mixed predictions. There is a substantial literature on quality disclosure, where firms can choose to either honestly and credibly announce their quality or else not provide any information at all. This scenario is a subset of the case I examine, where firms can also choose to misrepresent the attribute. A stark result from the early literature focusing on quality disclosure in monopoly (see Grossman [1981] and Milgrom [1981], as well as Jovanovic [1982] and Stiglitz [1975]) is that in the absence of disclosure costs any firm, regardless of quality, will choose to honestly disclose for strategic reasons. The argument is simple: a firm with quality greater than the mean quality of firm types that choose not to disclose will find it improves profit to disclose; iterating on this logic, only a firm with the very worst quality will then be indifferent between disclosing and not. Recent papers such as Board [2009] have shown that competition can increase or reduce the incentive to disclose; intuitively, disclosure may allow firms to benefit from differentiation. When the unknown attribute is the price, Ellison and Wolitzky [2012] show that strategic obfuscation of prices is optimal in oligopolistic competition with search costs, while in the model of Gabaix and Laibson [2006] the incentive to suppress pricing information about product add-ons is non-monotonic in the level of competition.

The empirical literature on quality disclosure has typically found that firms are strategic in disclosure, and that competition actually reduces the incentive to disclose.[1] Mathios [2000] finds that lower quality firms profitably obfuscate their attributes: prior to mandatory nutrition labelling, low-fat salad dressings advertised as such while high-fat dressings do not advertise, and after labeling high-fat dressings experienced a relative loss in sales. Zhe Jin [2005] shows that greater competition between HMOs reduces voluntary disclosure and leads to greater obfuscation of qualities. Misrepresentation of pricing is profitable even in a fairly competitive online environment: Brown et al. [2010] empirically explore the conditions under which shrouding a particular attribute of a good—the shipping price, when the good is sold through eBay—is profitable for sellers; Ellison and Ellison [2009] find evidence that obfuscation of prices by an online retailer raise profits by making consumers less informed.

There is a recent strand of empirical work closely related to this paper that examines how product misrepresentation changes with market structure and the regulatory environment. This work focuses on the airline industry, where the true quality of a flight is its actual flight time, while the quality the firm represents to consumers is the predicted flight time. Forbes et al. [2011] show that misrepresentation is strategic in the airline industry: airlines with the incentive to do so adjust on-time performance for flights whose late time would exceed a threshold, in order to reduce the proportion of late flights and increase their ranking. In a more recent paper, Forbes et al. [2017]

[1] See Dranove and Zhe Jin [2010] for an overview of the literature.

show that airlines increase reported performance by lengthening scheduled flight times on routes where they compete with entrants who also make quality reports. In other words, in response to competition airlines do not decrease flight length, but only increase *projected* flight length, resulting in more on-time or ahead of schedule flights. This idea that competition can lead firms to costlessly claim lower quality and thereby better the "optics" of their actual service finds a counterpoint in Zhe Jin [2003], where mandatory disclosure in competitive markets leads restaurants to actually improve their hygiene scores. This motivates the central research question of this paper: does competition lead firms to disclose download speed more honestly, or to misrepresent by claiming even faster speeds to grab consumer attention? And if disclosure becomes more honest, do firms revise their claimed download speed downwards or invest to increase the actual speeds?

The second research question this project focuses on is whether product misrepresentation leads to consumer misallocation across internet plans. A recent strand of empirical literature on consumer choice has focused on whether consumers make *ex ante* correct choices of subscription plans—be they health plans, cellphone plans Björkegren [2015], etc.—given their observed usage. The idea is that since most plans entail both an upfront tariff and a usage fee, one can back out whether a consumer has chosen the wrong plan by observing if they would have spent less money by choosing a different plan given their observed usage of plan services. Nevo et al. [2016] find that within the ISP they have data from, most consumers choose the lowest-cost internet plan given their observed data consumption. However, their research is not focused on explaining variation in the fraction of consumers making the "wrong" choice. Whether or not changes in misrepresentation due to competition change the degree of misallocation is an important question for policymakers interested in non-standard welfare effects of mergers.

3 Data

Since 2012, the Federal Communications Commission (FCC) has been collecting information at a household level to track ISP service quality as part of its Measuring Broadband America (MBA) program. The most important number the FCC provides statistics on is what I have termed the *promise ratio*: the average ratio of consumers' actual to purchased speeds across households at the same ISP.[2] The FCC updates information on the distribution of promise ratios at each ISP every year to help consumers make more informed choices about their internet options. The FCC emphasizes the promise ratio in its releases partly because the majority of consumers rate both reliability and download speed as the most important features of an internet plan. Prior to this program, there was no government data for tracking whether ISPs were providing the speeds they claimed; however, there were third-party testing sites that measure households' speeds, allowing a household to verify its actual internet quality but only after signing up.

[2]See https://www.fcc.gov/general/measuring-broadband-america

The main dataset is an unbalanced panel of MBA testing data. Units are observed for a maximum of four months spread over 3 years: April 2012, and September 2012, 2013 and 2014. Within each month, the plan that the consumer is subscribed to is verified with the ISP, and measurements are taken every two hours of the actual download speed, upload speeds, latency, video streaming rates and connection speeds to various popular websites (in later samples). The ratio of the measured download speed to the ISP-verified download speed is the main dependent variable of interest.

Using the FCC testing data, I first divide actual speed by claimed speed for each unit in each hour during which measurements are taken. I then trim the 1% and 99% quantiles to remove outliers. Finally, since the measurements are all taken for residential broadband subscribers and most usage occurs between 7pm and 11pm, I average the promise ratios across these peak hours within a month. This average forms the main dependent variable, and varies over time within a household across months during which the household is part of the sample. In Figure 1 I plot the empirical distribution of the promise ratio in the first month of the sample (April 2012) for 4 different ISPs: each ISP has a substantial fraction of households receiving less than the promised speed, but this fraction varies across ISPs.

With regards to sample balance, households continually drop out of the sample and new households are added in on a yearly basis. Since I do not know the criterion for adding in households, nor do I fully model the process by which households might exit the sample, I will instead show estimation results from both the balanced and unbalanced sample.

I use the Current Population Survey's (CPS) Internet Use Supplement to recover the fraction of individuals subscribing to satellite internet in 2010 and 2011. This fraction will form the basis for constructing the cross-sectional dummy variable indicating which counties are affected by the new satellite launch. While satellite internet was available prior to the launch of the new satellite in mid-2012, the maximum advertised speed was 1.5Mb/s and actual speeds were closer to 0.8Mb/s—not high enough to stream video smoothly.[3] To construct the treatment dummy, I classify treated counties as those in the 75th percentile or higher of satellite subscribers in 2010 and 2011. Figure 2 shows the distribution of the fraction of satellite subscribers across counties, in counties for which I have data.

To the panel data and treatment data I add supplementary data from the National Broadband Map (NBM). The NBM provides geographic data, measured half-yearly between 2010 and the middle of 2014, on which ISPs are available in each census block and on what speeds those ISPs offer. By matching units in the FCC panel to their census block, I can control for time-varying measures of terrestrial competition for each unit.

There will be two instruments for the treatment dummy: the fraction of houses that are prefabricated in a census block group, and the frequency of severe weather events. Information on

[3]Measuring Broadband America report 2012

the housing stock, including the fraction of mobile homes in a census block group, is recovered from the American Community Survey (ACS) 5-year sample. Information on the history of severe weather events at a county level comes from the National Oceanic and Atmosphere Administration (NOAA). The severe weather instrument is then constructed as the fraction of days during which the county is affected by thunderstorm winds. Finally, data on the daily quality of internet providers at a city level for 2010-2014 comes from Ookla, a large, free, online internet speed testing website. I use this data to verify that severe weather does indeed have an effect on the quality of wireless internet.

Summaries of variables for each of the four months that units are observed is available in Table 1. Note that there is a steady increase over time in both the promise ratio and claimed download speeds. The maximum advertised download speed is also increasing but not by as much as the actual download speeds individuals subscribe to. The average number of terrestrial competitors for a household barely moves over this time period—most individuals have a choice between only two providers—but there is a roughly 10% increase in the average number of competitors offering very fast internet by the end of the sample. Sample means of quantities such as housing density, the fraction of housing comprised of mobile homes, and the median house age that only vary cross-sectionally, will vary over time in Table 1 due to selection in the set of units that are in the sample as units drop out and the panel is refreshed.

4 Modelling and Estimation

The main specification for understanding the effect of the entry of the satellite competitor on the promise ratio is

$$y_{ijct} = \sum_{t>1} \delta_t \text{Treatment}_c \times D_t + \beta' X_{ijct} + \alpha_i + \alpha_{jt} + \epsilon_{ijct}, \quad (1)$$

where i indexes a household, j an ISP, c a county and $t = 1, \dots, 4$ one of the four months a household can be present in the sample.

The coefficients of interest are the δ_t, which measures the effect of being in a county exposed to the new satellite competitor at different time windows after the launch. y_{ijct} is the log of the promise ratio, and Treatment_c is a dummy taking a value of 1 if the consumer is in a county where satellite is a feasible option. The specification allows for time varying, ISP-specific changes in the promise ratio and variation in the average promise ratio across units. X_{ijct} includes the number of terrestrial competitors a unit has access to besides the ISP it is subscribed to, as well as the claimed download speed a unit is paying to receive.

As the literature makes clear, the sign of δ_t is an empirical question. In **?**, airlines increase their predicted flight time in response to competition while not improving the actual flight time; in my setting, the equivalent response would be to reduce the claimed speed consumers are receiving while maintaining the actual speed, leading to an apparent improvement in the promise ratio from competition but no actual change in quality. If some consumers are "naive" and take the claimed download speed to be true, then ISPs may have even greater incentives in the presence of competition to exaggerate and induce consumers to switch, leading to a reduction in the promise ratio. Finally, ISPs may also have the incentive to upgrade the quality of their service, improving the actual speed while maintaining claims. I will therefore run an auxiliary regression of the same form as Equation (1) where the dependent variable is the claimed download speed.

4.1 Identification

Insofar as the differential change in the mean promise ratio in treated counties can be attributed to a different composition of ISPs—who may be upgrading their national network differentially—the α_{jt} dummy will control for it. Moreover, since only within-household variation in the promise ratio is used, the effect within a treated county of treatment on the promise ratio cannot be attributed to consumer-side selection (e.g., if only lower promise ratio consumers were dropping service to switch to satellite). That is, neither firm-side or consumer-side differences in ex ante distribution across treated and untreated counties should pose a threat to identification of the competition effect.

In the background of the reduced form specification is an entry game: ISPs enter a small geographic region (a census block, or even a housing unit), choose how much to invest to provide a certain quality, and supply information—potentially false—about what quality can be provided to consumers. Firms also experience local shocks in their ability to maintain their promise ratio. Meanwhile, consumers can switch between providers, upgrade their nominal speed by paying more, or both.

The satellite-treatment methodology is intended to address a particular source of endogeneity: that of reverse causality from changes in the promise ratio to entry where entrants react to negative shocks to a competitor's ability to provide service by entering the market. The satellite launch is not endogenous to any local shocks. However, the ex ante fraction of satellite internet subscribers, which is used to construct the treatment dummy, may be endogenous to county level shocks. Ex ante difference of means tests across treated and untreated counties in the log promise ratio, number of competitors, number of competitors offering more than 50 Mb/s, and the maximum advertised download speed are presented in Table 2. The table suggests there are no ex ante difference in key observables between treated and untreated counties; however, the tests do not rule out differences in unobservable local shocks across counties, which I address below.

If there were ex ante unobserved negative shocks to competitors' promise ratios in some counties, then individuals may have substituted into satellite, leading these counties to be classified as treatment counties. There are then two options: if the shocks are persistent, then it implies treated counties' non-satellite providers are on a downwards trajectory. Any improvement in their promise ratios after treatment would therefore be a lower bound on the actual treatment effect. If the shocks are not persistent, then any improvement in treated counties' promise ratios may simply be mean reversion. To address this possibility, I instrument for the treatment dummy using variables that are likely not correlated with transitory negative shocks to competitors' promise ratios, but which determine the viability of satellite in a county. The instrumentation strategy also has the benefit of dealing with attenuation bias, which may arise because of mismeasurement/random non-response in the CPS.

The first instrument is the historic frequency of severe thunderstorms in a county. The location and duration (in days and hours) of severe weather events are reported by NOAA and come in many forms: tornadoes, thunderstorm wind, hail, etc. Cloud cover and heavy, prolonged rain are considered by experts to interfere substantively with wireless internet signals. In Table 3, I use data from Ookla Speedtest.net on to show that thunderstorms have a differential negative effect on several measures of internet quality for wireless versus fixed internet.[4] In particular, there are significantly more tests run during thunderstorms for wireless internet subscribers and latency—a measure of the responsiveness of real time interactions including gaming and voice calling—also increases differentially for wireless internet. In Table 4 I use testing data that does not provide measures of responsiveness, but which has more observations, download and upload speeds, and includes satellite internet subscribers. From this table it is clear that during thunderstorms, wireless and satellite internet both suffer a differential hit to both their download and upload speeds, with a larger magnitude decrease for satellite internet (although the coefficient is not significant.)

Counties with historically high storm activity are therefore less suitable for satellite simply because they will experience longer, more frequent service disruptions. Since the reliability of internet service is reported by consumers to be the most important feature of a plan, severe weather helps determine which counties are affected by the entry of the broadband satellite, while also being exogenous to transient shocks to fixed broadband providers' promise ratios.

The second instrument is the fraction of housing stock comprised of mobile/manufactured homes. In Boik [2017], the author finds that "households dwelling in manufactured/modular homes, conditional on the value of the home, are 9% less likely to adopt wired broadband ... [this] result cannot be explained by these households having lower disposable income to spend on internet access because such households are 20-67% *more* likely to adopt satellite broadband compared to households in non-manufactured homes, even in regions where there is no wired broadband provider." The result that households in mobile homes are much more likely to subscribe to satellite due to a higher fixed cost of setting up wired broadband provides cross-county variation in satellite suitability. Characteristics of the housing stock do not change quickly, and so are likely exogenous

[4] A disclaimer for this table is that since there are very few observations in the testing data for satellite internet, I rely on the effect of weather on wireless broadband (e.g., 3G and 4G) as a proxy.

to the types of transient shocks to competitors' promise ratios that have the potential to bias the treatment estimate.

5 Results

5.1 Promise Ratio

Regression results from the main specification Equation (1) are provided in Table 5 for the unbalanced panel. The first three columns provide results from standard fixed effects regressions without instrumenting for the treatment; these models suggest that the effect the satellite competitor is to raise the promise ratio of incumbents by about 1.7%. When the treatment dummy is instrumented for by the weather and housing stock instruments the estimate of the treatment effect increases by an order of magnitude. With date fixed effects, the entry of the satellite competitor increases the promise ratio of households at incumbent ISPs by 23.0%, while with an ISP-date fixed effect the treatment effect is 31.7% although it is significant only at the 10% level with standard errors clustered at the county level. Effects are significant for the time period immediately following the launch in September 2012.

Results for the balanced panel are presented in Table 6. The results are broadly similar in magnitude for the first time period post-satellite launch, but indicate that the increase in the promise ratio remains high even through September 2013 before attenuating in September 2014. This runs counter to expectations: one might imagine that if a low-promise-ratio household does not experience an improvement in speed, then they will switch providers and drop out of the sample. This dropping out would imply that the treatment effect in the unbalanced sample may be an overstatement of the effect; however, the opposite seems to be true. Given that attenuation still eventually occurs even in the balanced panel, it seems relatively safe to conclude that attenuation may not be within only 18 months time, but does happen eventually.

The regression specification takes care of selection at the household and ISP level, and the instrumentation strategy should deal with unobserved shocks to the promise ratio that may be correlated with the treatment dummy. These estimates provide an unbiased estimate of the effect of competition on the promise ratio; as competitors enter, firms reduce the negative misrepresentation of their products, but only in the short run.

I provide additional support for the idea that the improvement in the promise ratio is due to competitive pressures by running the regression separately for households exposed to more and less ex ante competitive environments. In Table 7 and Table 8, I that for households who already have an outside option offering speeds greater than 50 Mb/s, the introduction of the satellite competitor does not significantly affect the promise ratio of the incumbent; meanwhile, for households that do not have a high-speed alternative, there is a significant improvement in the promise ratio.

Since the promise ratio can be improved by either reduced the claimed speed or by increasing the actual speed, I provide an auxiliary regression where I split the sample by whether or not households experienced an increase in speed. By restricting the sample to only households that have no change in claimed speed and running Equation 1, I can verify whether any part of the change in the promise ratio is due to investment in improving the actual quality of service. Running separate regressions where claimed speed and actual speed are the dependent variables in an analogue of Forbes, Lederman, and Wither [2017] is not feasible in this environment since claimed speed changes can simply be the result of households' choices to change plans. Results are presented in Table 9; while the significance is low due to small sample sizes, it seems that the subsample of individuals who do not upgrade experience larger increases in the promise ratio after treatment than the sample of individuals who do upgrade. These results suggest that improvements in actual speed are an important channel through which competitive effects propagate.

Lastly, I check whether or not the increase in competition affects ISPs' census block level maximum advertised download speed. Since this maximum advertised download speed could reflect either increased claims or the result of substantial investment, I check whether competition has an effect on the maximum advertised upload speed as well. Intuitively, if attracting new households through stronger claims without improving actual speed is the goal, then since households tend not to care about upload speeds one might expect an increase in download speed only; however, when actual investment occurs, both download and upload speeds tend to improve. Results are presented in Table 10, and largely show null effects, suggesting that neither claims nor intensive investment in new infrastructure is occurring in response to competition. Note that these results are not inconsistent with those in previous tables showing a positive effect of investment: an ISP can increase the speeds of its current plans to a certain point without installing new infrastructure by, for instance, prioritizing their transmission on its network, but to offer 100 Mb/s where before the maximum was 25 Mb/s requires physical investment in new infrastructure.

Taken as a whole, the results suggest that the mechanism present in **?**—where firms react to competition by changing their claims but not their actual performance—is not operational here. Instead, firms increase their actual performance and do not reduce the claimed speed. This behaviour suggests both that misrepresentation is strategic insofar as its degree changes in response to market structure, and that changing only product claims in response to competition, while inexpensive, does not dominate actually improving service as a strategy.

5.2 Extensions

In this section I turned to the question of whether misrepresentation leads to consumer misallocation across plans. I construct two tests to address this question, based on how a household's consumption of content (in megabytes or gigabytes) varies with both their actual and promised downloaded speeds. While both exercises provide suggestive evidence of misallocation, neither constitutes a formal test.

Intuitively, consumption choice entails an extensive and intensive margin: consumers first pick a plan, and then choose how much content to consume. For instance, a household might want to be able to stream two Netflix videos during peak hours at a 480p resolution, and understands that 25 Mb/s should be sufficient to do so. Suppose the household purchases a 25 Mb/s plan based on the advertised "up to" speed, but only receives 15 Mb/s. Compared to a household that requires 15Mb/s, and who was promised and receives 15 Mb/s, the first household should consume more if they learn slowly or cannot switch easily. This follows because consumption does not just depend on speed, but a combination of preferences and speed, and the first household has exhibited stronger preferences for online consumption.

The first test of misallocation checks to see whether households who buy speed X and receive $Y < X$ consume more than households who purchase speed Y and receive Y. I select households whose promise ratios are within 0.02 of 1.0, and plot their monthly peak consumption in MB against their actual speed. I then match each household in this original sample with any households that receive the same actual speed (within 2%) as that original household, but with promise ratios less than 0.98, and plot the same line for those households. The results are in Figure 3, and show that for low and moderate speeds the households with promise ratios less than 0.98 consume substantially more despite receiving the same speed. I test for differences in consumption by speed quantiles and verify the difference is significant for low speed quantiles but insignificant at high speeds in Table 11.

It is possible in the previous cross-sectional exercise that though low-promise-ratio households are consuming more, they are actually satisfied with the service they are receiving. If they are satisfied, then one would not expect these households to receive an especially large boost to consumption following a marginal improvement in actual speed compared to a regular-ratio household receiving the same improvement. My second test for misallocation is of whether low-promise-ratio households are indeed satisfied with their service. For each household in the unbalanced panel, I select only observations where they are subscribed to their own within-household minimum modal plan speed—that is, I look at consecutive observations where a household does not upgrade or downgrade plans—and check whether they are satisfied with their current speed using the following difference-in-differences specification:

$$\log(c_{ijt}) = \gamma_j \log(\text{actual speed})_{it} + \theta \times \mathbf{1}[PR_{it} < 0.9] \times \log(\text{actual speed})_{it} + \alpha_i + \alpha_{jt} + \epsilon_{ijt}.$$

c_{ijt} is monthly consumption during peak hours in megabytes, and $\mathbf{1}[PR < 0.9]$ is a dummy taking a value of one if the promise ratio is lower than 0.9. If $\theta > 0$, then individuals with lower promise ratios receive an added boost from speed improvements.

Results are presented in the first two columns of Table 12 and indicate a significant differential effect of having a low promise ratio on the consumption boost from an increase in speed. The third column allows the baseline increase from an improvement in speed to vary based on whether the initial speed is low or high; the differential effect of a low promise ratio does not change much in magnitude, although the significance decreases. The last column checks for whether or not the boost in consumption from added speed is purely mechanical with a placebo test. If an increase in throughput simply allows background software or processes that communicate with servers on the internet to run more frequently this would show up as an increase in both downloads and uploads; I find no evidence for the effect of download speed on upload consumption, suggesting that the increase in consumption for low-promise-ratio households reflects household decision-making.

Without knowing the complete choice set of each household—in particular, what speeds are available and importantly the promise ratios of each speed—it is difficult to know whether or not a household is making their optimal choice. However, the second test in particular provides strong suggestive evidence that households with lower promise ratios seem to not be at their optimum.

5.3 Robustness

In this section I provide a number of robustness checks on the main results having to do with the promise ratio. These robustness checks are run using the balanced sample.

First, as shown in Figure 1, some ISPs actually provide speeds greater than what they promise to some measure of households. In Table 13, I run the same baseline regression but restrict the sample to only households with promise ratios less than 1 for the entire sample. The positive, significant coefficient remains, although the magnitude is decreased to about 20.5%.

Next, to construct the satellite treatment dummy I code regions in the 75th percentile or higher of ex ante satellite subscriber shares as being treatment groups. In Table 14, I experiment with changing the cutoff level. The sign remains the same in most specifications, although for lower cutoffs the significance disappears, partly due to lower estimated coefficient magnitudes. This result is not unexpected given that by lowering the cutoff, more and more counties that were actually not treated are included in the treatment group, so that the average improvement in the promise ratio after treatment is diluted.

Finally, I experiment with different controls for the terrestrial competitors. As shown in Table 7 and Table 8, having a high-speed competitor is an important determinant of the promise ratio and has important interactions with the treatment effect. I include various controls in Table 15 including the number of competitors that are national brands, and the number of competitors with medium, fast, and super fast download speeds. The inclusion of the different controls does not significantly move the coefficient on the treatment effects.

6 Conclusion

In this paper I analyze whether competition leads firms in the market for internet service provision to disclose product attributes more accurately. The paper connects to a rich literature on how competition affects firms' incentives to disclose product attributes; I expand on this literature by considering the case where firms can misrepresent their products by disclosing inaccurate information. I find that the effect of an additional high speed competitor is to raise the ratio of actual to claimed download speed by between 23 and 31 percent. Furthermore, I find that this effect is driven by improvements in the actual speed, and not simply reductions in the claimed speed.

I also provide preliminary evidence on whether firms' choice to misrepresent their products leads to misallocation of consumers across plans. If consumers correctly perceive true attributes even when firms misrepresent then misrepresentation is simply an efficient, cheap-talk equilibrium. I find weak support for the idea that misrepresentation led to misallocation of consumers across plans by showing that their data usage increased in response to the reduction in misallocation and increase in actual speeds.

Substantial work remains to be done on the topic of how market structure affects efficiency through the channel of misrepresentation. Given that Forbes et al. [2017] finds that competition leads to weaker claims but no improvement in service, it is necessary to write a theory model that can accommodate both behaviours—weakening claims or improving service—as equilibria, perhaps depending on parameters. Understanding how strategic misrepresentation can lead to lower investment in service quality is understudied even in the disclosure literature, which typically does not model an endogenous investment in quality.

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A Figures and Tables

Table 1: Means by Date

	Apr2012	Sept2012	Sept2013	Sept2014
Number of units	6790	6102	5245	4493
Promise ratio	0.9945845	1.0036044	1.0428606	1.0377242
Promise ratio 7pm-11pm	0.9696025	0.9722008	1.0098240	1.0018899
Download	15.09796	16.93083	23.33167	31.79436
Upload	4.196571	4.447210	5.864187	8.561023
Max adv. down	46.90078	48.08522	55.61642	64.21735
Max adv. up	4.691047	4.706396	7.028915	10.730917
Num. competitors	1.185095	1.189382	1.204331	1.196963
Num. comp. down > 50Mb/s	0.4344138	0.4250468	0.4572835	0.4705476
Housing Density	893.5948	871.8765	748.2968	713.7691
Frac. of Housing Mobile	0.04078837	0.04268021	0.04700761	0.04748228
Median House Age	39.66746	39.58704	38.55341	38.70599

Figure 1: Promise Ratio ECDF at 4 ISPs

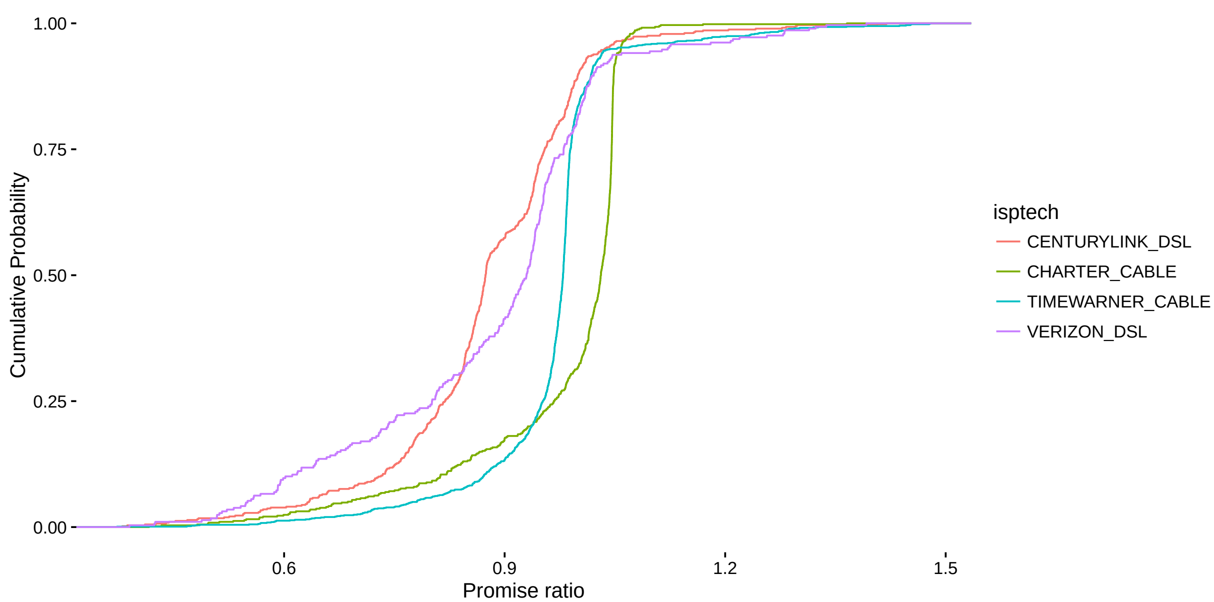

Figure 2: Ex Ante Satellite Subscription Shares

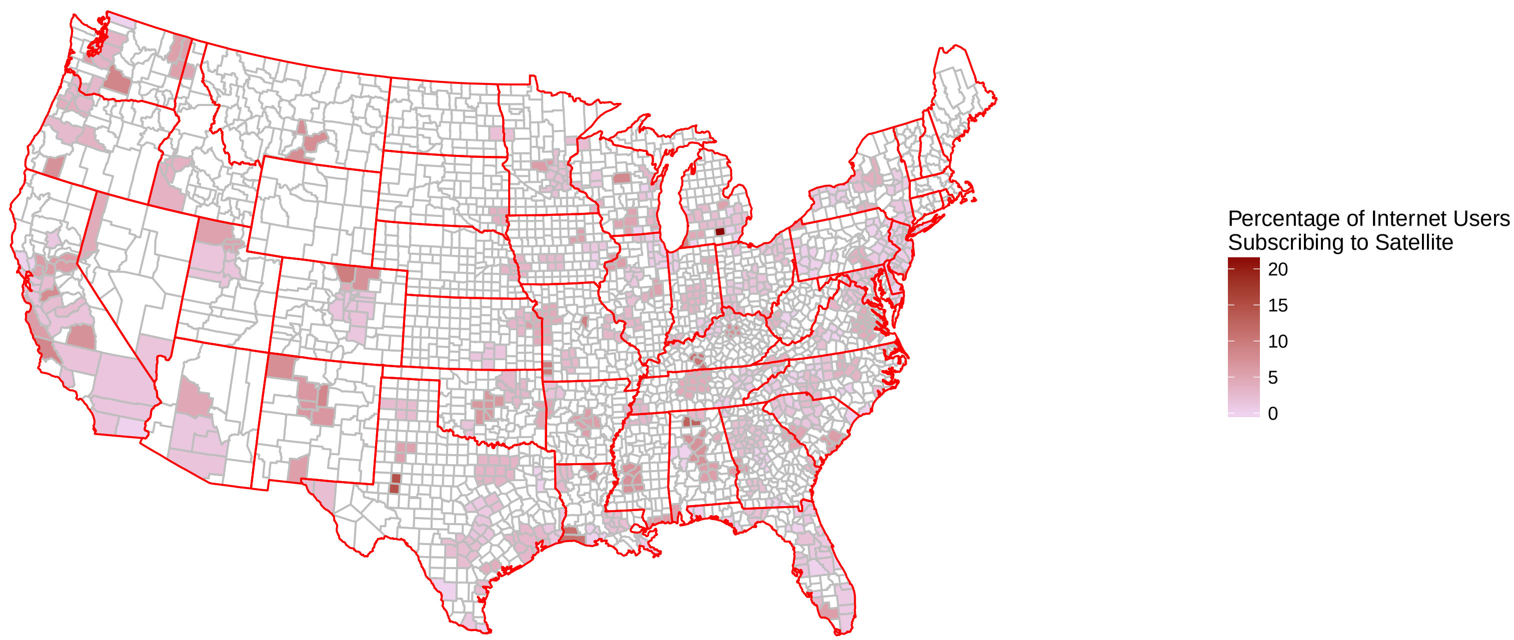

Table 2: Ex Ante Differences Between Counties

Variable	Untreated	Treated	ttest	pvalue
Promise Ratio	0.959	0.967	-0.898	0.370
Number of Competitors	1.122	1.136	-0.221	0.825
Num. Comps. >50Mb/s	0.348	0.372	-0.564	0.574
Max Adv. Down	43.178	47.779	-1.364	0.174

Table 3: Effect of Thunderstorms on Internet Quality

	Dependent variable:				
	log(Total Tests)	VOIP Quality	Jitter	Packet Loss	Latency
	(1)	(2)	(3)	(4)	(5)
Thunder × Fixed	0.003	-0.396^{***}	0.478^{***}	0.134^{***}	-0.388^{***}
	(0.002)	(0.036)	(0.073)	(0.012)	(0.133)
Thunder × Wireless	0.104^{***}	0.650^{***}	0.573^{**}	-0.423^{***}	3.140^{***}
	(0.007)	(0.139)	(0.278)	(0.046)	(0.506)
ISP×City FE	✓	✓	✓	✓	✓
Date FE	✓	✓	✓	✓	✓
Observations	184,961	184,961	184,961	184,961	184,961
R^2	0.689	0.756	0.760	0.785	0.795

Note: *p<0.1; **p<0.05; ***p<0.01

Figure 3: Household consumption differences by claim fulfillment

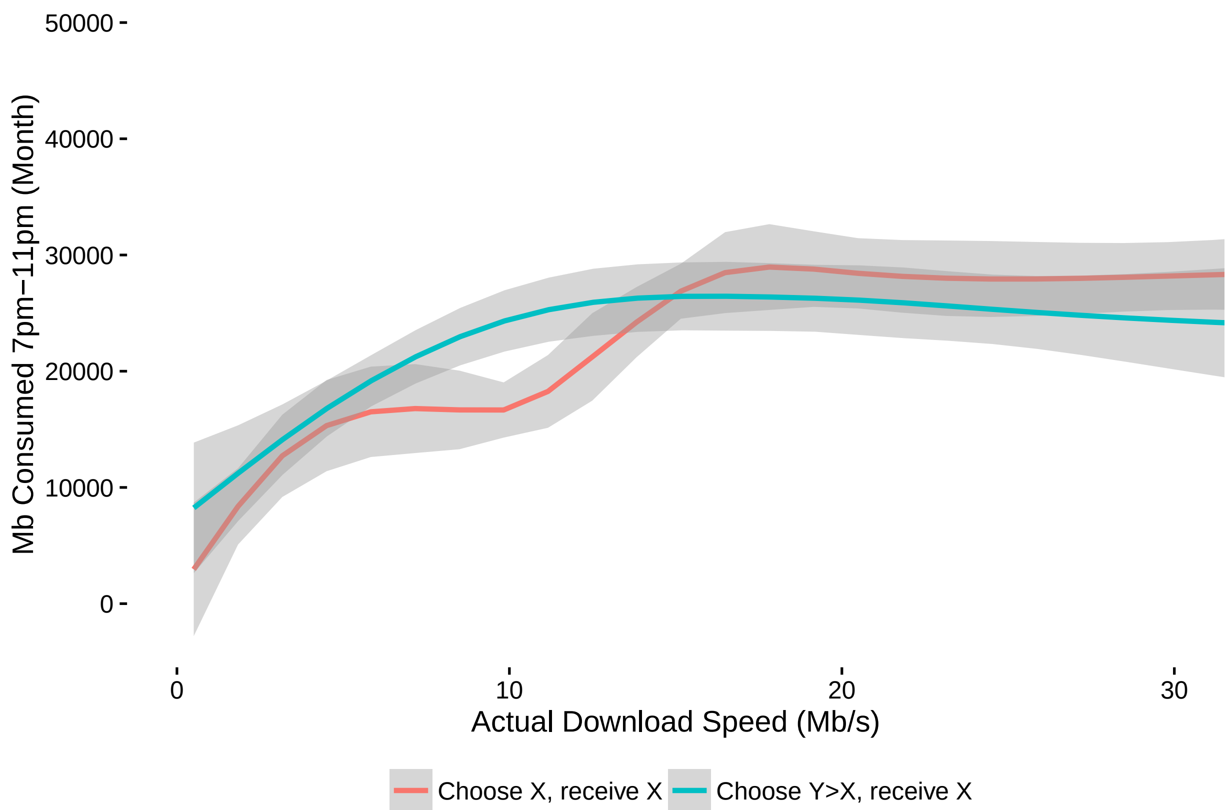

Table 4: Effect of Thunderstorms on Internet Speeds

	Dependent variable:		
	log(Total Tests)	MB/s down	MB/s up
	(1)	(2)	(3)
Thunder×Fixed	−0.009***	0.151***	0.172***
	(0.0005)	(0.007)	(0.005)
Thunder×Satellite	−0.007	−0.194	−0.200
	(0.021)	(0.296)	(0.216)
Thunder×Wireless	−0.009***	−0.123***	−0.072***
	(0.001)	(0.015)	(0.011)
ISP×City FE	✓	✓	✓
Date FE	✓	✓	✓
Observations	11,292,923	11,292,923	11,292,923
R^2	0.886	0.784	0.791

Note: *p<0.1; **p<0.05; ***p<0.01

B Model

[TO BE ADDED]

Table 5: Main Results: Unbalanced Sample

	Dependent variable:				
	log(Promise Ratio)				
	OLS			*IV*	
	(1)	(2)	(3)	(4)	(5)
Treatment $\times t_1$	0.013	0.017	0.017**	0.230**	0.317*
	(0.011)	(0.012)	(0.008)	(0.116)	(0.182)
Treatment $\times t_2$	0.062***	0.035**	0.020**	0.162	0.096
	(0.012)	(0.014)	(0.009)	(0.121)	(0.175)
Treatment $\times t_3$	0.056***	0.026	0.004	0.037	0.085
	(0.016)	(0.017)	(0.010)	(0.133)	(0.192)
log(down)	−0.009	−0.028***	−0.060***	−0.028***	−0.056***
	(0.008)	(0.008)	(0.004)	(0.005)	(0.005)
Ncomp	0.00001	−0.002	−0.006	−0.002	−0.0001
	(0.012)	(0.012)	(0.007)	(0.010)	(0.010)
Unit FE	✓	✓	✓	✓	✓
Date FE		✓		✓	
ISP × Date FE			✓		✓
Observations	16,627	16,627	16,627	10,940	10,940
R^2	0.642	0.647	0.680	0.628	0.646
Note:			*p<0.1; **p<0.05; ***p<0.01		

Table 6: Main Results: Balanced Sample

	Dependent variable:				
	log(Promise Ratio)				
	OLS			*IV*	
	(1)	(2)	(3)	(4)	(5)
Treat $\times t_1$	0.010	0.009	0.012	0.221*	0.248*
	(0.013)	(0.013)	(0.012)	(0.125)	(0.134)
Treat $\times t_2$	0.067***	0.040***	0.026*	0.326**	0.317*
	(0.013)	(0.015)	(0.013)	(0.141)	(0.182)
Treat $\times t_3$	0.049***	0.022	0.002	0.084	0.095
	(0.014)	(0.016)	(0.014)	(0.141)	(0.182)
log(down)	0.005	−0.015	−0.042***	−0.016***	−0.041***
	(0.009)	(0.009)	(0.008)	(0.006)	(0.006)
Ncomp	−0.009	−0.010	−0.012	−0.011	−0.011
	(0.013)	(0.014)	(0.013)	(0.012)	(0.012)
Unit FE	✓	✓	✓	✓	✓
Date FE		✓		✓	
ISP × Date FE			✓		✓
Observations	8,369	8,369	8,369	5,459	5,459
R^2	0.544	0.550	0.603	0.486	0.553
Note:					*p<0.1; **p<0.05; ***p<0.01

Table 7: Main Results by Initial Competition: Unbalanced

	Dependent variable:			
	log(Promise Ratio)			
	(1)	(2)	(3)	(4)
Treatment×t_1	−0.058	0.293**	−0.096	0.260*
	(0.137)	(0.145)	(0.195)	(0.239)
Treatment×t_2	0.125	0.230	0.167	0.064
	(0.167)	(0.151)	(0.207)	(0.243)
Treatment×t_3	−0.234	0.109	0.025	0.020
	(0.215)	(0.156)	(0.341)	(0.222)
log(down)	−0.012	−0.043***	−0.046***	−0.057***
	(0.008)	(0.008)	(0.008)	(0.008)
Ncomp	−0.012	0.023	−0.011	0.007
	(0.014)	(0.015)	(0.018)	(0.015)
Init.Comp.$\geq$ 50 Mbit/sec	✓		✓	
Unit FE	✓	✓	✓	✓
Date FE	✓	✓		
ISP × Date FE			✓	✓
Observations	4,293	5,285	4,293	5,285
R^2	0.618	0.549	0.671	0.595

Note: *p<0.1; **p<0.05; ***p<0.01

Table 8: Main Results by Initial Competition: Balanced

	Dependent variable:			
	log(Promise Ratio)			
	(1)	(2)	(3)	(4)
Treat $\times t_1$	0.050	0.237*	0.190	0.278*
	(0.228)	(0.130)	(0.339)	(0.153)
Treat $\times t_2$	−0.066	0.407***	0.041	0.382**
	(0.232)	(0.156)	(0.351)	(0.191)
Treat $\times t_3$	−0.361	0.190	−0.220	0.202
	(0.231)	(0.156)	(0.367)	(0.205)
ldown	−0.0003	−0.042***	−0.023**	−0.061***
	(0.009)	(0.009)	(0.010)	(0.009)
Ncomp	−0.012	0.017	0.002	−0.018
	(0.017)	(0.017)	(0.020)	(0.019)
Init.Comp.$\geq$ 50	✓		✓	
Unit FE	✓	✓	✓	✓
Date FE	✓	✓		
ISP $\times$ Date FE			✓	✓
Observations	2,412	2,990	2,412	2,990
R^2	0.506	0.419	0.607	0.494

Note: *p<0.1; **p<0.05; ***p<0.01

Table 9: Households that don't upgrade have larger treatment effect

	Dependent variable:			
	log(Promise Ratio)			
	Unbalanced		Balanced	
	Never upgrade	Upgrade	Never upgrade	Upgrade
	(1)	(2)	(3)	(4)
Treat $\times t_1$	0.461*	0.199	0.183	0.169
	(0.278)	(0.235)	(0.250)	(0.191)
Treat $\times t_2$	0.099	0.090	0.299*	0.232
	(0.250)	(0.223)	(0.181)	(0.202)
Treat $\times t_3$	0.008	0.059	−0.117	0.055
	(0.272)	(0.246)	(0.257)	(0.204)
log(Down)		−0.058***		−0.044***
		(0.006)		(0.007)
Ncomp	0.014	−0.004	−0.002	−0.015
	(0.024)	(0.014)	(0.024)	(0.016)
Unit FE	✓	✓	✓	✓
Date FE		✓		✓
ISP × Date FE			✓	
Observations	4,578	6,362	1,283	4,176
R^2	0.733	0.572	0.622	0.558

Note: *p<0.1; **p<0.05; ***p<0.01

Table 10: Effect of treatment on advertised speeds

	Dependent variable:			
	Unbalanced		Balanced	
	log(Max Adv. Down)	log(Max Adv. Up)	log(Max Adv. Down)	log(Max Adv. Up)
	(1)	(2)	(3)	(4)
Treat $\times t_1$	0.294	0.195	0.083	0.117
	(0.322)	(0.445)	(0.348)	(0.485)
Treat $\times t_2$	0.486	0.409	0.070	−0.254
	(0.310)	(0.428)	(0.366)	(0.510)
Treat $\times t_3$	0.742^{**}	−0.551	0.571	−0.830
	(0.342)	(0.472)	(0.366)	(0.511)
Ncomp	−0.015	0.200^{***}	−0.022	0.215^{***}
	(0.018)	(0.025)	(0.025)	(0.035)
Censusblock FE	✓	✓	✓	✓
ISP × Date FE	✓	✓	✓	✓
Observations	10,940	10,940	5,459	5,459
R^2	0.969	0.937	0.954	0.910
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 11: Household consumption differences by claim fulfillment

	Act. Down < 6	Act Down $\in [6, 12)$	Act. Down $\in [12, 20)$	Act. Down ≥ 20
receive Claim	$10,981.640$	$16,161.590$	$27,651.970$	$28,707.040$
receive $<$ Claim	$17,021.660$	$23,326.350$	$25,806.440$	$26,200.570$
p. value	0.00002	0.0002	0.268	0.300

Table 12: Household differential consumption increase

	Dependent variable:			
	log(MB Downloaded)			log(MB Uploaded)
	(1)	(2)	(3)	(4)
log(Act. Down Mb/s)	0.240**	0.294***	0.234*	0.094
	(0.097)	(0.100)	(0.136)	(0.106)
log(Act. Down Mb/s) $\times\mathbf{1}[Down < 10]$			0.117	
			(0.179)	
log(Act. Down Mb/s) $\times\mathbf{1}[PR < 0.9]$	0.047	0.057**	0.052*	−0.003
	(0.029)	(0.029)	(0.030)	(0.031)
Unit FE	✓	✓	✓	✓
Date FE	✓		✓	
ISP × Date FE		✓		✓
Observations	12,014	12,014	12,014	11,515
R^2	0.833	0.836	0.836	0.869
Note:			*p<0.1; **p<0.05; ***p<0.01	

Table 13: Robustness: only ratios ≤ 1

	Dependent variable:	
	log(Promise Ratio)	
	(1)	(2)
Treatment$\times t_1$	0.171	0.205**
	(0.146)	(0.100)
Treatment$\times t_2$	0.149	0.199
	(0.158)	(0.221)
Treatment$\times t_3$	−0.212	−0.158
	(0.168)	(0.214)
ldown	−0.044***	−0.061***
	(0.011)	(0.012)
Ncomp	−0.003	−0.006
	(0.019)	(0.020)
Unit FE	✓	✓
Date FE	✓	
ISP × Date FE		✓
Observations	5,133	5,133
R^2	0.693	0.705
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table 14: Robustness: different sat. share cutoffs

	Dependent variable:			
	log(Promise Ratio)			
	(1)	(2)	(3)	(4)
Treatment $\times t_1$	0.100	0.202	0.138	0.440*
	(0.125)	(0.179)	(0.105)	(0.289)
Treatment $\times t_2$	0.138	0.176	0.086	0.144
	(0.146)	(0.216)	(0.114)	(0.246)
Treatment $\times t_3$	0.099	0.120	0.067	0.143
	(0.136)	(0.210)	(0.116)	(0.304)
ldown	-0.053^{***}	-0.054^{***}	-0.054^{***}	-0.056^{***}
	(0.005)	(0.006)	(0.005)	(0.006)
Ncomp	0.001	-0.003	-0.001	-0.001
	(0.010)	(0.011)	(0.009)	(0.010)
Cutoff	0.5	0.6	0.7	0.8
Unit FE	✓	✓	✓	✓
ISP × Date FE	✓	✓	✓	✓
Observations	10,940	10,940	10,940	10,940
R^2	0.676	0.662	0.680	0.629
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 15: Robustness: different terrestrial competitors

	Dependent variable:			
	log(Promise Ratio)			
	(1)	(2)	(3)	(4)
Treatment$\times t_1$	0.317*	0.320*	0.317*	0.316*
	(0.182)	(0.182)	(0.182)	(0.182)
Treatment$\times t_2$	0.094	0.092	0.093	0.093
	(0.174)	(0.175)	(0.176)	(0.175)
Treatment$\times t_3$	0.086	0.094	0.086	0.078
	(0.193)	(0.192)	(0.192)	(0.191)
log(down)	-0.056^{***}	-0.056^{***}	-0.056^{***}	-0.056^{***}
	(0.005)	(0.005)	(0.005)	(0.005)
Ncomp National	-0.011			
	(0.012)			
Ncomp $> 10Mb/s$		0.009		
		(0.008)		
Ncomp $> 50Mb/s$			0.026*	
			(0.015)	
Ncomp $> 1Gb/s$				-0.030
				(0.029)
Unit FE	✓	✓	✓	✓
ISP $\times$ Date FE	✓	✓	✓	✓
Observations	10,940	10,940	10,940	10,940
R^2	0.646	0.645	0.646	0.646
Note:			*p<0.1; **p<0.05; ***p<0.01	